Patuxent Science Symposium
75th Anniversary
October 13-14, 2011

This transcript is Part 2 of 4 and features the speakers listed below for the 75th Anniversary of the Patuxent Research Center. They talk about their work and experiences while working at Patuxent.

Patuxent Science Symposium
Thursday
Session Moderator: David Trauger

David Trauger introductory: Dr. Susan Haseltine

David Trauger:
Our first speaker this afternoon is Dr. Susan Haseltine.

Sue has recently retired from a long and distinguished career that began at Patuxent as a scientist. And included several prestigious positions on the way to becoming the Chief Biologist for USGS and then Assistant Director for Biology at USGS.

I had the opportunity of having Sue work for me at Patuxent, and later I had the opportunity of working for her at USGS. So, you know, on the way up you've got to be careful of the people you're working with, they may end up being your boss. And it certainly came true with us. But we, Sue and I had a great relationship for all these years. And I'm delighted to welcome you to the stage, Sue.

Dr. Susan Haseltine:
Well, thanks Dave. Okay, well the sort of subject that Matt assigned me was the role of contaminants in the general arena. But I very early on in thinking about this topic determined that I was not qualified to do that talk. And so because, you know, I spent a number of years here at Patuxent doing contaminant research, and then went off and did research in other institutions in the Fish and Wildlife Service. And eventually became an ARD for Refuges and Migratory Birds and Private Lands in the Fish and Wildlife Service. And then joined Matt, my research colleagues...,

As we were becoming NBS and in fact, I was hired back into research when NBS had enjoyed a budget increase and the support of the Secretary of Interior and all this stuff. And so I accepted this job as an executive manager in the new agency, thinking I was going to get back to research and we were going to do all these wonderful things. And the next week, Congress cut our budget by 20%. And so, needless to say, and many people have too alluded to it this morning, we really trimmed down a lot of expectations that we had about doing research for conservation.

So, and you know, through all of that I know there were many stresses and strains in the whole organization. But Patuxent has really survived in its original orientation and its

programs have remained strong. And to me one of the reasons for that is that Patuxent has always had this connection with an impact and a use of its information that so many people have talked about this morning in migratory birds and in other things. And basically, while Patuxent has been an excellent science institution, it has focused on science that led to efforts in conservation and led to other scientific efforts that helped conservation.

And so I'm..., and that's, you know, that's not an earth shaking revelation since it's always sat within the Department of Interior. And once we skyrocketed the animal damage control people to agriculture, you know, the biological mission in all of the heroes in the Department of Interior has a conservation focus.

And so Patuxent has always been on the science side of developing new information and techniques and efforts in addressing conservation. But it has always had that tight link to conservation and perhaps, as several people mentioned this morning, it was that link to the Migratory Bird Management Office which was such an integral part of the Fish and Wildlife Service effort for many years.

Perhaps it was other things, but this focus on payoff is something that is relatively unique in the research system as we knew it in the old Fish and Wildlife Service, and as we know it in the USGS today.

And so I'd like to kind of talk about how this developed because I think it's very important for not just the survival of science within the Department of Interior establishment, but also the use and influence of science in our society as we move forward into the 21st century.

And in many ways, the development of this science in the early years of Patuxent mirrored the development of the whole wildlife science field. And we were fortunate to have some of the early leaders like Dr. Dustman and Dr. Stickel that focused the effort within a framework of excellent science. And you've already heard presentations this morning and you'll hear many more this afternoon about the individuals who provided such leadership in this area.

But I regard Patuxent more than any other research institution that grew up through the Fish and Wildlife Service and landed in USGS as an engine for both more science and conservation efforts and programs within the service. And I'd like to explore this link a little bit more by talking to you about how Patuxent science has been central really to the development of the ecological sciences, the legal and political systems that we use in conservation across the country, and to wildlife and land conservation itself through the

Fish and Wildlife Service and other institutions.

So, starting with ecological sciences; you know instead of taking the focus of after World War II, you know much of wildlife science, much of natural biological science was really natural history based up until that period, very observational, very..., a lot of use of correlations and tradition. And really the evolution of that science, wildlife science and many other biological science towards ecological process and the ecological sciences, which is more a hypothesis and model-based, was mirrored here at Patuxent. And in many fields like environmental contaminants and endangered species and model development in migratory birds and habitat conservation, Patuxent really provided a lot of leadership. And you could say that we even, if you look at it from a whole institution base like I did when I was in USGS, we've gone too far into hypothesis and model-based work. And now we need to develop more things like the phenology network that's more observation-based because we have the tools to use those observations now.

So, but we really focused, while I was here, in moving from that observational base to more hypotheses. That meant we used much of the field research that was observation-based and tried to make it more experimental, we wanted to control experimental techniques. We dealt with the idea of time and space scale, you know, if you did an experiment in Massachusetts and the robins died, or you made that observation as was highlighted from Rachel Carson's book, what could you extrapolate to the rest of the continent? And so we got much more quantitative.

And in doing that, Patuxent really pioneered a lot of technique development and indicative development, and a lot of that's going to be talked about this afternoon in other talks; Woody Hill's going to talk, I'm sure, about some of the standardized testing that was developed.

And in using and in developing these techniques, we quite blatantly stole from many other fields in biology, especially medicine. I'm sure some of you are going to talk about the organophosphate era where we did that.

We made much more use of chemical techniques until a standard wildlife biologist like me got very familiar with terms like lipophilic and organometals and all of that stuff, and used the chemists that we had on staff.

And we really got used to the idea of taking this comprehensive approach; field and laboratory. All types of backgrounds contributing to our knowledge of how contaminants or habitat alteration or hunting, in the case of migratory birds, was having an impact on

individuals, on populations, and on continental species ranges.

That was kind of the context that we all worked in.

And so what I would say is that we really took a statistical approach to all of this when I first came to Patuxent. Or I should say when I first started my graduate work at Ohio State the DDT hearings had just gone on. And, you know, Bob Heath was kind of a hero of those DDT hearings. He was on the stand for a long time. And he was the first fulltime statistician that the Fish and Wildlife Service ever hired in the Service. Patuxent pioneered that approach.

And, to me, the trajectory of Patuxent has been, you know, building from that in that we sorted out correlations and cause and effect, geographic scale so that you could figure out if you were doing a study in Maryland, how you could extrapolate to the rest of the world. And really focused in on the population as the center of the information that we should be developing for our conservation colleagues in the refuge system and migratory birds.

So what about wildlife and land conservation? What has Patuxent contributed to that? Well, you know, some of it's really obvious. The whole contaminant program in the Fish and Wildlife Service started at Patuxent, and a little later the Fish-Pesticide Lab in Columbia, Missouri. The Endangered Species Program really grew up at Patuxent, and then became an operational program in the Fish and Wildlife Service.

And in many ways a lot of the urban wildlife stuff that was done here as part of the early efforts at Patuxent became the tenants of the Private Land Program that grew up in the Fish and Wildlife Service.

And so, as research programs and knowledge matured at Patuxent and then at other scientific places, they were made operational, or given a more management focus within the Fish and Wildlife Service.
And as we looked at new impacts that came up with this comprehensive approach out on the landscape, whether it was water (Harry Ohlendorf is going to talk about the drain water story in California) or on landscapes or with smelters (I remember a lot of Nelson Beyer's work on smelters, or dioxins that came up later) we ran those, all of those through the same system, and they too became operational.

And many of the approaches that started in the research arena; captive propagation, especially with endangered species or to look at cause and effects in contaminants became an operational possibility in managing species.

Remote sensing, the stories on that are region models; we heard that story this morning a little bit from Mike Erwin. And the monitoring story I don't think has come to fruition or will ever really end because monitoring is one of those things that neither in research nor in management will ever have enough money for, or will be able to decide what the important things are. But we certainly have sparked a lot of interest and effort from Patuxent.

But perhaps the most interesting arena that I think Patuxent had influence in is a legal and political one. And I think part of that, the reason that Patuxent has been so influential; Jerry Longcore and I were talking at lunch about some old lead shot stories, about getting it banned or putting new regulations in for substitute shot, which Patuxent had huge influence in over the years.

I think one of the reasons is that the management and the scientific staff at Patuxent have always remained true to what I call scientific rigor. They reviewed all proposals that came in, and tried to do the thing that was most applicable to the question that the legal and political community had to fill in the knowledge gaps. And when that information was available, have always said that scientists should interpret it first, and that they should be the ones that translate the findings to the legal and political community. And sometimes that's extremely uncomfortable.

And I'm sure almost everybody sitting in the room has their own story about meetings with assistant secretaries or senators or whatever, who are not happy about the facts. But, you know, in the end, those facts led us to the solutions that have stood the test of time. And that tradition at Patuxent I wish was as strong throughout the whole establishment.

And then finally, communication; I don't think that I would say that Patuxent has had a leadership role in that communication and the communication of science to the rest of the world. But it certainly has had people over the years who have had a marvelous opportunity for that. And I think that the tradition at Patuxent has been that if you have something important to say, you must communicate that. And you must communicate it in a way that's true to the scientific method and the scientific validity of your results. But you also must communicate it in ways that other people will understand. And I don't think there's any substitute for that in science. And I hope that all of the young scientists that are coming on at Patuxent have a lot more ability in that regard than we did as we came in, so, because that's one of the hardest efforts of scientists in an institution like Patuxent.

And there certainly are a lot of grand challenges for the future that the kind of

comprehensive approach and long-term approach that Patuxent uses will be extremely valuable in. Several people have already mentioned the whole issue of climate change, increasing human population which, of course, affects habitat in its quality and quantity.

And one of the other things is that I think Patuxent perhaps doesn't quite marshal its resources. It has many of them, but doesn't marshal them as well as in other areas as in disease and genetic manipulation. There are just more and more and more issues that are going to affect wildlife communities and populations on the landscape. With these two issues there is a tremendous core at Patuxent to really take a comprehensive approach with those two things.

And I know that the disease efforts, many of them, especially for warm-blooded animals, moved to Madison in the '80s and, you know, we didn't follow up on all of those studies like Barry Tarshis did on leucocytozoonosis in waterfowl and all that stuff.

But, disease and genetics are merging as a field, and I think Patuxent could make a big impact if they took that comprehensive approach.

So, my wish for the future would be that Patuxent continue in the strong science areas it has. And continue to take that comprehensive approach that emphasizes science, partnerships, and communication to bite off some of these big issues in society. And I think this is one of the institutions that will continue to have great value to our society.

Thanks.

David Trauger:
Any questions for Sue? Yes, Brad.

Brad:
(Nick: 23:16) gave us some comment regarding communication. I know there's internal elements and there's external elements with congress, you know, multiple types of communication. But I want to go back to about the first conversation I ever had with Greg Smith when he came here two years ago. He said he was so excited, this was his third center I believe, he served at.

David Trauger
Who's counting?

Greg:
Yes! And it was the first one where he had access to a built-in communication center like

a visitor's center, like the National Wildlife Visitor's Center where obviously we're hosting this two-day symposium. And that further communication capability to the public, you know, on Saturday many of you are going to be involved, talking to 3-year-olds, 80-year-old people coming here for the festival. And hopefully getting people to care more about the environment issues of the day, planting those conservation seeds that, you know, will maybe take over certain lives if they get turned on just the right way at this facility. So that's one really important way to communicate, at least to the public sector, that I hope you all think about taking as much advantage of it as you can.

David Trauger:
Great.

Dr. Susan Haseltine:
Could I follow up on that a little bit?

David Trauger:
Sure.

Dr. Susan Haseltine:
Just because this was Greg, and when I worked with Greg in Region 3 and in the refuge system as well, and it sort of spared me on this thing of the communication of research and managers, even within the Fish and Wildlife Service. You know, I worked for 15 years in the research establishment in the Fish and Wildlife Service and then I switched to management in Refuges and migratory birds.

And just a couple of quick vignettes if you think it's really, really good that communication partnership. The first week that I was an ARD in the Refuge System, I had to do a refuge inspection. Those are big formal things; they're like white glove things (unclear: 25:21). So I'm on the, I'm chairing the panel, and we go into this refuge and we're sitting there and I sit through the first day. It's like, "Okay, is your flagpole the right height?" And, "I see some paint that's chipping on your garage door." And, you know. And then I sat through the second day, and it's like, "Okay, you didn't open the gates early enough in the morning." And day three, it's a four-day review; day three, 3 o'clock in the afternoon, somebody says, "And how's your migratory bird populations doing?" Almost at the end of the reviews so, you know, you need... And then the other story that kind of put me in the idea that we need to do better is that first Christmas that I was, I was the..., one of my jobs was the Center Director for Northern Prairie. They always have a big Christmas party. So the first Christmas I offered to host it at my house. So I'm standing in the kitchen and some of the folks from Denver, the operational region were up including the regional director. And the regional director comes in the kitchen,

I'm taking the turkey out of the oven, and Galen looks at me and he says, "Dang, you're the first person from Region 8 that ever fed me a meal out of their oven. I'm going to have to pay more attention to research in the future."

So, those are just little vignettes that stick out in your mind. But it was amazing to me the number of issues that people in the refuge system had to deal with before they got to their primary mission.

And research and science is really an engine of change and, you know, change is always hard. So, we need to keep working on it.

David Trauger:
Okay, we move right along, but I can't resist putting another footnote on Sue's talk. I'm glad that she brought up as one of the big challenges of the, grand challenges of the future, increasing human population.

We are within hours, if not days of having the seventh billion inhabitant of planet earth, and on our march to nine billion by 2015. Think about the implications for land and water and wildlife, about that. And I think we, we haven't had enough focus on that, and we really need to focus on that.
I'm really pleased that you brought that up because the future's not what it used to be.

And we're going to be going through very rapid and dramatic change, and it's going to be largely driven by that increasing population.

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Session Moderator: David Trauger

David Trauger introductory: Jerry Longcore

David Trauger:
Moving right along, Jerry Longcore is our next speaker.

And he's worked with Patuxent for 39 years, both at Laurel, here, and he also was involved as a staff specialist in the Wildlife Division Office in D.C.

He's been the leader of our main field station, and he's done a lot of research on a number

of species up there related to waterfowl and other birds. But he's an authority on the American Black Duck, and he's going to focus on that in his talk today. Jerry.

Jerry Longcore:
While we're getting ready to talk about this topic of Black Duck, I just want to make sure that you all are field biologists, you know that. All you have to do is to count the number of ducklings out there, and if you can get the right number, you're on your way.

When Matt asked me to talk at, for this seminar, he wanted me to talk about the lead shot shooting tests and so forth. And I could have done that, because I think I'm the only person in the United States that has shot over 3,000 Mallards, without a permit!

So what I tried to do in this talk was look at some of the literature that came out of Patuxent, and then I want to tell you the person that put a lot of it together for the Black Duck was Ken Reinecke, and it's still available to us on the website.

And I looked at all the papers and who published what; a lot of the names I'm going to show you are people who published works. I know there's a lot of people behind the scenes whose names are not on papers. And as I told all the people down at Fish Pesticide Lab one time when I was a staff specialist and went down there was in their review, I said, "You know, you scientists are up front here, you're giving us all this good information and so forth." But I said, "Who empties your wastebasket?" And it was kind of quiet for a minute. And I said, "Every person at this lab makes this lab work."

So, I think that's the same thing for Patuxent. Every person at the lab, and Lynda Garrett was mentioned earlier. I can say from the field point of view, many, many times I'd send her requests, and she says, "I don't have it here but I'll get it." And she always did get the paper I needed.

So, there's a lot of people behind the scenes who's names you won't see, but they, they contributed to the Black Duck work.

So, let's see if this works here.

I want to, let's talk about history a little bit. So, I had to put a picture in here because it's tied in with a Michigan forester. It's my graduation picture in 1961. Now, at the same time coming out of that institution was another individual who I'll talk about later, was Warren Blandin. So, we'll talk about the Atlantic flyway reps and what they meant to the Black Duck research.

Now, we've got a lot of history, but to put it in a perspective for myself I had to kind of think back about, 'Well, where did we all come from?' 'What are our roots for Patuxent and work on the Black Ducks?'

Well, you've got to go back a long ways, and that's really the people who did all the work with finding out what birds ate because of the economic aspects of crops and so forth. So back in 1885, there was an Economic Ornithology Unit in USDA. Then in 1905, they actually named it the Bureau of Biological Survey, and that was still in USDA.

Moving on, in 1939, it was moved from USDA to the Department of Interior. Then in '56, the Fish and Wildlife Act and we had the Fish and Wildlife Service, which was also called the Bureaus of the Sport Fisheries and Wildlife.

Then in '93, of course, as was mentioned..., I like, I like the maelstrom up there of the picture of the National Biological Service. I always..., we always kind of kidded because I actually served on the Transition Committee as a field representative on that and made many trips to Washington to be involved in that discussion. And it was really a long and deep discussion and we came up with all kinds of names to try to name it. And NBS, what does that stand for? And some of the times when we meet, well, it means, 'No BS.' I'm not so sure.

And then when we got, like Gingrich I guess it was, didn't like us, so we got changed from the Service..., from the Survey to the Service. And if it wasn't for Senator (unclear: 33:35) in Ohio, who thought it would be a bad idea to redline the whole budget for the NBSO, because it looked a little bad if you let that many federal employees go. And they, they said, "Well, what are we going to do with them?" Well, they put us in the USGS as a new division, Biologic Resource Division.

So we survived that, I think, and we kept moving on.

But if you go to some of the old refuges; there's one in Maine, Moosehorn National Wildlife Refuge, you will find a sign on some of the old wetlands of the old biological survey signs, and this is one of them.

Now much was said about F.C. Lincoln, been mentioned several times, and he probably had as much to do with keeping Patuxent people employed as anybody. Because if we think of all the people who have worked with banding data, and come up with models and looked at survival and the banding recovery rates and reporting rates and so forth.

And this one of the early pictures of F.C. Lincoln that... There's a history behind this

picture; one of our colleagues that some of us worked with and others knew well was Dick Smith. And everybody knows Dick Smith as being somewhat gruff at times. And I know one time it was the time to clean out the office files in Washington and stuff was going into the circular file pretty rapidly. This came flying by me and I said, "Wow, what's that?" And I looked on the back and it was a picture of F.C. Lincoln, you know a little write up on the back in an old typewriter written with spaces and the old ink and stuff. And I said, "I'm going to keep that." And so I kept it. And so this is, this is the old picture of F.C. Lincoln that was salvaged out of the hidden office.

Now he was the individual that did a lot for the banding work. And as somebody mentioned before, and I came to the same conclusion, he really developed the concept of the flyways and, based on his banding of birds.

Also, when Howard Mendall, who was the unit leader at Maine retired, a lot of his old books and library was kind of put out on a table, you know, anybody wanted it they could take it. So I browsed through it, and I ran across the manual for bird banders. And this is actually a copy that was authored by F.C. Lincoln. And I actually have it with me if anybody wants to see it.

Now, what I've done is looked at kind of four major groups of studies, and then tried to tie the people with them. So I'm not going to say a whole lot about the studies themselves, but I want to give the credit to all those fine people and researchers who contributed to work that was somehow associated with Black Ducks.

First there was, of course, the contaminants. And you can't mention contaminants at Patuxent without mentioning Dr. Stickel, and one of the things that I think she served as a mentor to many others in that regard. And I remember my first paper that I got ready to send to the pesticide research coordinator, who Lucille was at that time, for review before it went out to the Journal. And I tell you when I got it back I just had to put it away for a few days. I was seeing red and blue and every color ink on it where it needed to be improved and corrected.

But I think that was one of the best things that could have happened to me at Patuxent, because she showed me that to write scientifically is not the same as just writing. You have to write clearly and succinctly and..., to get your point across. Because if you can't communicate, as was mentioned by Sue, where are you? You know, we've got to get the message out and get it out correctly.

The next major group of studies that were sort of basic ecology studies, some life history sort of things, habitat use survival, movements, energy requirement and so forth.

Another aspect, of course, was all of the work being done here at Patuxent with our statisticians and biometricians, survey, people setting up surveys and so forth.

And then the last is one that I kind of threw in there because throughout the whole Black Duck issue, there's been this underlying idea that somehow the Mallard was superior to the Black Duck. And so that was part of the issue that the population was declining. Behaviorally, they were saying that the Mallard was so dominant that it would take over a female Black Duck. And so that's why you got the hybrids and so on.

Well, if you look at the literature, you'll find out there's a paper out there that shows the male Black Duck has bred with 52 other different waterfowl species. So it's not too discriminatory.

And the other thing is, I remember John Langenbach, who used to be assistant director here; John used to say, "Well," he'd say, "you know, those Mallards," he'd say, "they'd mate with a snake if they could catch them in the (wheel well: 38:12)." So he didn't have much opinion, high opinion of them either.

So we're going to show you a little bit about the method of the superior Mallard, or the "Oh mighty Mallard," as John mentioned.

I put this in ten year decades; during the first 20 years of Bureau Biological Survey, which was so named in 1905, no specific Black Duck papers showed up in the 22 bulletins or 48 circulars that were published. But some of the authors are interesting. William McAtee; I saw another person mentioned Wallace Cook, who did a lot of good work; F.C. Lincoln, of course. And then there's a slug of others in there too; (unclear: 38:44), Cottam, and so forth.

And also two chiefs of the Biological Surveys; C. Hart Merriam and H. W. Henshaw, which of course, are the two labs here. And my office used to be in Merriam Lab, so I'm sort of attached to that one.

Now in the early days the bag limits were a pretty good size. In fact, I found in a publication that said that the hunters from the eastern shore said that when you get them flying you keep on shooting until you're out of shells. And that was kind of the way it was then. And there wasn't much enforcement either. And, in fact, there was even less enforcement up in the wilds of Canada.

I'm not going to say too much about that right now, but later we'll mention that again.

Well, in the '40s, it was a time of Ira Gabrielson; of course we have another building named after him. But he was really instrumental in a lot of the work. And he talked about, in a publication I'm going to show you the title of in a minute here, 'The program should be accompanied by restrictions on shooting sufficient to kill, limit to lessen the annual number of ducks put on the lake.'

So, even back in 1947, people were knowing the population was going down, and there was a call to do something about it.

And here it is, Ira Gabrielson and L. Day, who wrote a book on waterfowl. At this particular time he was writing science and documents about a federal aid program.

But the program for the Black Duck; and in that he kind of laid out what was happening habitat-wise and hunting-wise and so forth. And the irony of this whole business on the Black Duck research is that almost everybody, and when we get to '68, even the first symposium in 1968, almost everybody there at the symposium, Canadians and U.S. people, were agreeing that the underlying problem was harvest, or over-harvest.

But, there was also that a little bit of underlying thing about the Black Duck and the Mallard relationship, and so nobody did anything.

The other thing, if it would have happened then, a change in the regulations, we'd probably not ever have been..., had employment for as many years because the U.S., in the U.S. the federal government sets the rights to the..., and the states go along with that.

In Canada it's not that way. In Canada it's, the federal government there, Canadian Wildlife Service, is a player among all of the Provinces. And the Provinces have a lot of clout. So, if you want to change something, it's not so easy a thing up in Canada as it is in the states here. Not that it's easy in the states sometimes.

So in the '50s, a lot of work was done in the Chesapeake Bay, and a lot of done by R.E. Stewart. He published the distribution maps on the Black Duck. And during those years through the mid-winter inventory, which was validated at least as a good index, although it's not perfect, it was still a pretty decent index. Well, it was 500,000 to 600,000 Black Ducks, which is quite a few. But the trend over the time up to then was still downward. And this is the publication that shows the breeding range and the wintering range of the Black Duck that R.E. Stewart published in 1958.

And, of course, the Black Duck speculum is becoming an issue again because of the

Banding Office now is putting out some information on what, how to call a Black Duck hybrid. Whether it's a Black Duck-Mallard hybrid or Mallard-Black Duck hybrid or a Crossback or whatever it is. And it's based on is there any white in the leading edge of the speculum or any white in the trailing edge of the speculum, and some under the wing brown feathers. So, that's still being played out I think. We haven't heard the end of that.

Okay, so the long-term decline on the Black Ducks; Jerry Serie was involved. I put his pictures next to this slide because it shows what was happening to the Mississippi Flyway, the Atlantic Flyway, and the total flyways.

But, if you notice right at the end, there's a take, uptake in the number of birds in each one of these lines. And that's because of something that happened in the '80s, 1983; I'll show you that in a minute.

So, 1960 through '69, there's quite a bit of work done to see, in fact, if contaminants were involved. So they went out and looked at wings and looked at eggs. And Stewart did one of the nicest publications you want to find, if you want to find some historical good data on populations in the Chesapeake Bay, it's there. But, if you look at the acknowledgements in that paper, who is helping out at the back end of that was Fran Uhler and his microscope, identifying a lot of things; Neil Hotchkiss identifying things; Bob Mitchell; and then Lucille Stickel actually reviewed that 208 page tome. So, a lot of Patuxent people involved in a support role there.

Now this is 1983, I believe it is, and I looked through the pictures in that and realized there's probably at least 18 members of the staff here at Patuxent in '83 were doing Black Duck research in some level, in some way here at the center. So, there's a lot of people involved.

Just to mention a few: Mike Haramis did some great work on acid rain; John Sincock actually did some work in the Currituck Sound, North Carolina for example. You know Jim Nichols's work, Mike Conroy and the model, did some contaminants work. Bernie Mulhern did some of the pesticide chemistry work and so forth. Even statisticians, I've got a few statisticians like Chris Swarth did some background work there. So, a lot of good people doing a lot of good work.

Now, I don't know about you but Fran Uhler was probably one of my best mentors here at the center. Coming in as a young biologist thinking he knew something, and going out one day with Fran to the Eastern Shore. I got my first crab cake by the way from Fran; he bought crab cakes for me, never had them before coming out of the Midwest. And then going along a windrow of vegetation, and him picking up anything I showed him, he

can give me the life history of it. And whether it was increasing or decreasing or what was going on with that particular plant or snail or whatever it was.

So in 1968 then there was a symposium held. And Ed Addy led all the discussions, and he was the flyway biologist representative at that time, Atlantic Flyway. And if you were in the Atlantic Flyway, you had to do or know something about Black Ducks because that's the flyway that has most of the Black Ducks.

Some of the other people from Patuxent that gave talks there were Kahler Martinson, Al Geis, and Robert I. Smith. Lucille gave a talk on the environmental pollution aspects. Frank McGilvery at the time was working on the imprinting of Black Ducks and Mallards in nest box to cut down on predator losses. And Ed Chamberlin was listed in some of the aerial surveys in the Bay. And there were seven other biologists, including myself who were there. In fact, I rode over to the conference with Ed Addy, which was interesting because, again, I was just fairly new and learned a lot from him.

Here's that 'august' group right there; Ed Addy..., the picture's here because Ed's in it, but these other interesting characters too; Art Hawkins, of course Mississippi Flyway rep for years, Horton Jensen and Al Smith, and of course Jerry Stiller, I remember Jerry Stiller did a lot of work on canvasbacks. And the big discussion was why is it when the Redheads lay eggs in Canvasback nests (because they're parasitic on Canvasbacks) that the egg of the Canvasback will roll out of the nest and the Redhead egg will stay in it. Think about that.

So we go into a long discussion on it. And it turns out, if you think about it, the Redhead egg is much more spherical than the other, and so..., and the Canvasback egg is a little more elongated and one end's bigger. So if you flip any one of them, you can flip one and it'll go 'whoop' but a round egg won't.

So after a long discussion, he said, "I never thought of that." I said, "Well, you ought to think of that."

Now through the '70s..., '70s up through '79, more work was done on contaminants; mercury, DDE, the DDE studies, the eggshell thing was a big issue and the fix on productivity. And that was the reason that we got involved in the Black Duck-DDE study, which I'll talk about in a second.

Also, more work was being done then with harvest-based on banding data. McGilvery's imprinting work. Ken Reinecke, although I don't think he was with the Service at that time, was finishing up his doctorate work at Maine, was doing work on foods and

energetics on female Black Ducks.

But in '76, it was kind of a pivotal point for me because I was sent to Maine to work on the breeding biology of Black Ducks and Ron Kirby was sent to New Jersey, to work in New Jersey on the Patuxent to study the wintering ecology. And so the Service was getting serious about finding out what was going on with the Black Ducks.

Now the Black Duck-DDE study; I put Bill Stickel's picture in here because Bill is another one of my mentors because he served in a role in Washington as a staff specialist in the interim period as a temporary before I went to Washington. And he told me to burn them, Bill wrote me a poem about that thick, of the whole aspects of the staff work in Washington, and he told me how to survive down there. And it became sort of a thing I would look at every once in awhile because he said one of the first things you do when you go to Washington, he said, "You get to know all the secretaries of the people you deal with, you get to know them by their first name." And he said, "If you do that, and you treat them with respect," he said, "you'll receive the same." And he couldn't have been more right.

Ruth Sunshine, who was Dr. Hester's secretary, when I left she came to the going away party. And she said, because we had this thing, I would try to send that memo to her that would be perfect, with no errors in it. She was always trying to catch something and call me up and say, "Jerry, I think you've got a comment..." And so..., but when I left she came and she said, "You know, Jerry," she said, "you're the only one of these things I've ever gone to." I thought, "Wow." I think I, I tried hard and I think she appreciated that.

But anyway, the Black Duck study was..., they up in the bird yard Bob Keith and the crew up there, Clyde Vance and Jim Spann and others did some really great work on showing effects of DDE and shell study. And they tried to do it with Black Ducks. But Black Ducks are not Mallards, and they react a little differently, they behave a little differently. And so they couldn't get it to work so they could get eggs and so forth out of the Black Ducks.

And so they said..., they came to the Wetland Ecology Division. And I think this is part of the, like Patuxent works. If you've got enough people there of different disciplines and backgrounds, if it's not going to work here you look around and see what else you've got in your staff. And they said, "Well, what do we need to do to get them to breed?" And I said, "Well, how about building 40 pens for them?" And of course that was like, 'Wow, that's quite, you know, a bit of work and a lot of money.' And so I said, "Well, if you want them to breed, let's see if we can do that."

So I figured out the spaces they'd needed and we built 40 pens. And that's where people like Tom Hobbs and Bill Henson and others on the staff, and Henry, you know, they all contributed. And we built 40 pens. And the results were 40 pairs, 36 of them nested, which..., so that was pretty good.

And that was the first year, and then we incubated those in incubators. And the second year we let the hens incubate it. So we tried to follow up on the work of Derrick Ratcliff in the '80s, where he showed that his sparrow hawks eggs were all thrown out of the nest. So we said, "Well, does this happen? Does shell cracking increase if the hens incubate?" And sure enough, there was a four-fold increase. So it sort of validated, the last validated the field stuff.

And I think that was another thing that Patuxent has been famous for, is not being satisfied with one avenue, but a couple of avenues of validation.

And, of course, the other story is the shells and the DDT hearings. Sue mentioned it; the rest of the story, Bob Heath was really pummeled by this hearing examiner, Mr. Sweeney. And I was to testify, the next person on the docket along with D. Porter, he was after me. And to show you the support we got from our people at the lab, I went to Dr. Dustman and I said, "You know, I'm not on trial here," I said, "and D isn't on trial here and Bob isn't on trial here." I said, "I'm not going to take that, I'm not going to testify." And Dr. Dustman said, "Fine." He didn't give me any guff, he just said, "Fine." Talked to the Solicitor of the Interior and we cancelled the hearings for two days. And when I went back, of course, the first thing I had to answer to was Mr. Sweeney. And he said, "Who told you not to testify?" And of course I was a lot younger than and probably not as circumspect as I am now. And I just looked at him and glared right back and said, "Nobody." Just about like that. So we went on to testify.

But it just shows you there's a lot of things that go on as a biologist, some of them are fun and some of them are not so much fun.

Now studies got going on the wintering grounds also, and here's Mike down there catching, looking at some dead Brant, they had a real bad winter. A big banding coup. And I don't know all the rest of the people, I think one of them might be Matt and one of them might be Liz Bell, but I know one of them was Walter Quist, who did a lot of work with Matt, but with Ron Kirby.

So into the '80s, Warren Blandin, here's his picture from 1961, he was the Atlantic Flyway rep. More work was done with contaminant effects, not only..., not just the

eggshell thing but other organofluorines and so forth. Brian Cain was involved with some of it.

More aspects of the breeding biology; Reinecke-Ringelman studies were going on. A lot of stuff on just basic ecology like how do broods move, what's the survival of hens coming off the nests, what's nest tentativeness of hens and so forth. A lot of work like that.

Jim Goldsberry was involved in a lot of this and doing some telemetry flying for us. Danny Stotts, Dave Krementz. Some of the work that Dave did on the wintering, yeah the wintering grounds, but it was really a breeding season in Chesapeake Bay showed that nest initiation times and clutch sizes had not changed. So that, you know, you couldn't say it was that that caused the decline of the Black Duck and so forth.

And of course Mike Conroy started out with some of the work that he did on validating the mid-winter inventory for one thing. Then he had some telemetry studies on the Chesapeake Bay.

In the '80s, another call for action, you had bad days for Black Ducks. Still, the population until that time was still on that downward trajectory. And Clint Townsend was a leading conservationist in Maine, and he put this out, you know, and showed that there were still people interested in, you know, out there.

About that time in '86, the Black Duck Joint Venture came together under the North American Waterfowl Plan. And that was another key part of this whole issue. The Black Duck was named a species of international concern. They set up a population goal of 350,000 for the wintering index. Both Fish and Wildlife Service, U.S., and Canada, put up money.

And then another thing that was really critical, I think, was a technical committee was established. Here again, the scientific rigor aspect of it came through to review research proposals. And that was really important.

Now lots of fieldwork was done in the '80s; all kinds of pictures here putting on radios, looking at vegetation, sexing and aging. You'll notice we have quite a nice crew of helpers in our banding crew one year.

Jim Ringelman was a key person; he was assisting me as a biologist. He did his PhD and did a really great job of all the work.

Here's some field camps, we had field camps a lot of places. And look at the picture at the bottom, bottom left. When it said in there, "Use business attire to come to this conference," I almost dug out my old sheepskin tie to say “This is my business attire as a field biologist.” But my wife talked me out of it.

So, it's kind of summed up in a few things about science. Science is organized common sense where many of beautiful thirty was killed by animal fat. And here was theory that..., this was published in '87, this individual and colleague, 'Increasing Mallards, decreasing American Black Ducks: Coincidence or cause and effect?’ Well, not so much.

Emerging out of these facts turned out to be that Black Ducks are not dominated by Mallards. They're... Black Ducks are as aggressive as Mallards and defend their territories and females. They're not excluded from fertile wetlands. Brood sizes are not different in Mallards in fertile or infertile wetland. And hunting mortality of Black Ducks can be additive to the natural mortality. And, you know, you might lose a few ducklings (like this is one of Mike's big pictures in his studies, this acid rain).

The '90s through '99, acid rain studies by Mike. Now we're in the USGS era, in '93. Primus did a lot of good works done on the bay, what makes ecology survive and so forth. Ehrlich did work, Dan McAuley, John Sour. And lots of work done in banding analysis and population work with Serie, Conroy, (Tremitz: 58:16), Nichols, the crew, the whole crew.

We continued to challenge the Mallard method; the Mallard were superior, the studies.

The second symposium was held in 1990; again, the same, a lot of the same academy of people working on things. They all contributed papers and so forth.

And this was kind of a key thing, I think, that came out that on this concept of the threshold of adaptivity of hunting losses. Whatever their point is, it may be easy to exceed on the breeding grounds or the areas where the birds may be particularly vulnerable. Harvest rates early in the season, early in the season on adult females and young in the breeding and staging areas could be severe. That was Anderson and Vern. Of course, Anderson was here and Vernon was here; a couple of our colleagues.

So, one of the last studies we did on Black Ducks was to kind of validate that last statement, that is, is it severe, is hunting really important? It seemed to be always..., really it should have been, we should have known this a long time ago. So we went out, our colleagues put together a sample at Amherst Point, Nova Scotia; we radio--marked

birds at Missisquoi Refuge in Vermont and at two sites up at Esquimaux and Kamouraska in Quebec in collaboration with people; Jean-Francois Giroux from the University of Montreal in Quebec; L'Université du Québec à Montréal.

But this is the breeding ground; this is where they could be vulnerable. And so we're looking right here over the eastern shore of the St. Lawrence with our telemetry system, picking out birds. And here's the Yamaska Delta, the beautiful..., lots of habitat there for ducks, nesting and breeding and staging. So, that's the kind of habitat you had to look at.

Eight-six percent of all the confirmed mortalities from the hunting studies in those two years were hunting-related. And the study was an open and shut case that yeah, hunting does take birds. Duh!

A little bit more work from 2000 to 2011; some work that Mike did with..., on..., in the acid rain work and so forth. And I think there was some work in Anacostia, habitat restoration by Hammerschlag. We did some more basic ecology work; Goldsberry did a lot of the flying for us on telemetry work. Jorde did some work; Dennis Jorde did some work with Black Ducks. So we had a lot of folks still working on Black Ducks. Serie, of course, was the Atlantic Flyway Rep, more band analysis, and we continue to challenge the Mallard.

So the third Black Duck symposium was held in 2002. And these individuals contributed papers. And one of the things, there was a key paper that came out, not by us but by Gene Lorton from the Chesapeake Bay, was that the Black Duck does not tolerate people as well as the Mallard does. And that's really obvious. He showed it with his data and we knew it from our field experience that you could look at a Black Duck over there, and if it sees you it's gone. The Mallard will sit and he'll watch you for a while, swim around. They're just; they're just not that spooky.

And this is a picture from one of the blinds up in Quebec.

Now, so what happened was that it was okay to sleep with a hypothesis, but you should never become married to one. And this is what was in a lot of people's minds. That this dominance of the Mallard was the reason that the Black Duck was declining, and we didn't believe it.

So, to kind of get to the end here; in all science, error precedes the truth. And it is better; it should go first then last. So after the Black Duck harvest was reduced, Black Duck population increased, in spite of the Mallard population increase. Competitive exclusion for these two species, at least, was discredited and decreasing harvest, increasing Black

Ducks, cause and effect.

That's what was happening in 1990 through 2008 in the harvest. And see what, we reduced our bag limits, wow. And this is what happened since, since 1988 through 2008, the population trajectory is up for the Black Duck. So it's sort of a success story, I think.

And this is what kept us going in the field, these frosty mornings with the frost on the vegetation, a wading drake waiting for his hen to come off the nest.

David Trauger:
Thanks, Jerry.

Another reason is sample sites. You can measure and do a lot more in an hour in a skimmer colony, which you see here, than you can do in days looking for ospreys.

Also, here's a picture of mixed colony; egrets, spoonbills, cormorants. And all of those are nests; they're close to the ground in some colonies. You see you can do a lot of sample sizes. Great as far as from a productivity standpoint in the number of birds you can sample, also an ease of collection.

Patuxent Science Symposium
Thursday
Session Moderator: David Trauger

David Trauger introductory: Dr. Gary Heinz

David Trauger:
Our next speaker is Dr. Gary Heinz.

Gary's been working at Patuxent since 1969, and has been involved with numerous contaminant studies, especially the heavy metals and..., like mercury. And Gary...

Dr. Gary Heinz:
I think everybody knows what a metal is. Aluminum is a metal and it’s light, lead is a

metal and it's heavy. That's close enough for you guys.

There's a couple of things I want to talk about in my presentation. One is, just like Jerry's presentation, did you know the number of names of scientists who worked for decades, their careers on Black Ducks, to bring things to fruition? That's one thing. The other thing is what Sue mentioned, we're not just collecting data. There has to be a payoff. There has to be a conservation benefit. It's not just an intellectual exercise. So with our contaminant work and with the heavy metal work, this, I think, was borne out a lot.

We've worked with lots of heavy metals; we work with mercury, with thallium, cadmium, chromium, but I want to focus just on one because of time constraints. And it's going to show the history of how it takes a lot of people a long time to solve a problem. And there's one thing with heavy metals that's different, Jerry, than DDT. You know, when DDT was banned in 1972, nature started to break it down and degrade it.

Heavy metals are elements. You can't break that atom down. They're around for the long haul. It's just a matter of whether they resurface and with... The one heavy metal I'm going to talk about is lead. You'll see how it kept coming back again and again and again to haunt conservation.

The first problem that we started working on, Jerry alluded to, is lead shot mortality in waterfowl. So hunters went out. When they shot at ducks, pellets came out of their shotgun shells. If those pellets were lead, which was all there was back then, birds would pick them up (I think most of you people know the lead shot story) they would pick them up either as grit or mistake them for seeds. And what happened was birds were found dead from lead poisoning.

Sue mentioned this and alluded to it and so did Jerry, in all of our contaminant work there is a cycling between field research and lab research. And the lead shot situation might be, well, are birds dying from lead poisoning?' Yes, they are. 'How many pellets does it take?' Let's feed them in the lab. Okay, let's go back and see how many birds have that many pellets in their gut. Well, it's a different species. Well, there's different diet. Let's bring them into the lab.

So it's a cycling of laboratory and field research, both of which are complimentary and essential to unraveling some of these things.

Way back in 1951, a scientist here, Don Coburn, tested the toxicity of lead to Mallard, 1951. In the '60s field collections were made. This is Lou Lockland, Lou Sileo..., Lou Sileo. You can look at a bird and find out does it have the lesions that are characteristic

of lead poison.

Jerry Longcore was very modest. A lot of the work that he did with his comrades, including work on iron shot, which would one day become a substitute for lead, this was absolutely critical in determining that lead shot was poisonous and that you could substitute something for it and still be able to go out and hunt ducks.

But in the ‘70s, look at all these different studies that were conducted, including the ones that showed that things like Sora Rails and Mourning Doves, that they could pick up lead shots too by picking around for seeds and could mistake them for seeds. Or they could pick them up intentionally as grit.

So other species in addition to waterfowl were found to be affected. And again, look at all the hosts of scientists that worked on this for part of their career.

In the ‘80s, people like Barnett Rattner and Jim Fleming and Chris Funk went, "Okay, there are certain species more sensitive than others." Matt Perry and Mike Harris went on the Chesapeake Bay, and Chris Franson who is a veterinarian here, they found that you could just look at the blood cultures of a bird. You don't have to kill the bird and find out that bird is being subjected to dangerous levels of lead.

Noel Snyder, who got the photo credit for this, I should have mentioned, up here; Noel found the first California Condor that was poisoned by fragments of lead bullets. Not lead pellets from a shotgun shell but lead bullets. And Hank Pattee and Noel wrote about the effects of lead poisoning, secondary poisoning on eagles and condors that would get a dead deer that was shot with a bullet and no one found it, or a jackrabbit or something of that nature. So predators, predatory birds could be, and scavengers, could be affected.

In 1991, here came the payoffs though. In 1991, you know, lead shot was banned for all waterfowl hunting. The estimates say that up to 3 million waterfowl were saved every year; Jerry; that was the benefit of the work that you and Ralph and so many of those other people did. Three million birds saved per year with the banning of lead shot.

But work went on. Nelson Beyer and Lou Sileo and others published a paper on how do you prove by looking at a dead bird that that bird actually did die of lead poison versus something else? They really pinned it down.

Then there were shooting ranges where people had shot skeet for decades, there were millions and millions and millions of lead pellets out there that Mourning Doves and other birds could pick up.

Then the big question, okay we can't use lead anymore. What kind of pellets can we put in shotgun shells that will kill waterfowl humanely? And companies came up with them. But they had to be tested and the data had to be examined to say this stuff, whether it's steel or bismuth or tungsten, these are other heavy metals, they will not be poisonous. And our own Barnett Ratner was in charge of reviewing all these reams of data, proving that these new shots were, in fact, not toxic.

Well, after lead was banned, we thought, 'Okay, is that it for lead?' Well no, there was a little interlude where we found out that a huge amount, I don't really trust this figure, I was telling somebody at lunch this is what I read in a book and I think it's got to be an error, but they get..., A lot of people told stuff today that wasn't 100% true. So what the heck!

But lead from automobiles, whether it was that much or less, got into ditches and in fields and so forth. People like Nelson and Chris Grue looked at songbirds. Don Sparling is here, he looked at tadpoles and the frogs that come from them.

And yes, lead fortunately was banned. It was phased out; it was banned utterly by EPA in 1996.

Okay, so got rid of the lead shot problem, got rid of the lead in tetraethyl lead in gasoline that can hurt wildlife. I guess we're finished with lead.

Well, no, we weren't really finished with it because it rose its head again in the '90s with lead problems in the Coeur d'Alene River in Idaho. One hundred and fifty or so dead Tundra Swans were found every year, poisoned by lead in the Coeur d'Alene River, Idaho.

The first thing people did, they looked for lead pellets. Couldn't find the lead pellets. They were not dying of lead shot that was deposited in the Coeur d'Alene River. But we did know that the swans and other birds, such as shown in this slide, were being killed in mine tailings. They had mined lead for a hundred years along the river, and all of that mud left over washed down to the river. So what's the first thing wildlife toxicologists would think of, a little bit of lead getting into the river, and the bugs are eating it and the little fish are eating that, and the big fish and bigger fish. So it's food chain magnification.

So Chuck Henny and Larry Blus had the idea, let's go out and look at something that eats big fish like ospreys. See if they eat lots of lead. This is a photo by Barnett Rattner. But

these folks looked at the osprey, they found out that ospreys had lead, but they didn't have a horrible amount, and their eggs were hatching. So, lead wasn't coming from the lead shot, it wasn't coming up through the food chain. So what was the problem? I mean, they're... the nests of the birds and the eggs were doing fine. Here's a little funny slide I took of an osprey nest in the Chesapeake Bay; we're floating an egg to see its development. There's the other osprey egg. But ospreys bring back pieces of paper and foam, and this, you know this....Going, 'What is an osprey thinking about!' Only thing I could figure is when the little ones hatch that they've got to have something!

Well, what Chuck and Larry Blus concluded was this, (you guys read the top thing by yourself); they thought these birds when they were on the bottom of, on the marshes along the Coeur d'Alene River, they're getting seeds and roots. And they are coincidentally, or perhaps purposely thinking it's seeds or grit that they need in their gizzard, they're picking up the mud that's in the Coeur d'Alene River along with the food.

Now, how did we know that that could possibly happen? Well, Nelson Beyer had done the studies showing how much sediment, ergo mud, do birds tend to pick up when they feed off of the bottom of marshes? So, we knew that it was technically possible for the birds to be dying that way.

But, we went back, we've got all these pens at Patuxent so we filled them up (these are other kinds of pens) with ducks and the geese. And we used as a surrogate for the Tundra Swan we used the Mute Swan; we got them from the Chesapeake Bay.

When we tested these we found out that the amounts of mud that the waterfowl typically consumed in the Coeur d'Alene River, coupled with the amount of lead that was in that mud, that is definitely what killed those birds. We were able to prove that with the laboratory studies.

Other people, such as Nelson and Dave Hoffman, found that it's not just the waterfowl. Because if you take Robins or Swainson's thrushes that live along the Coeur d'Alene River, you know, there's dried out areas, mud dries out and blows up on and through the wind. And these birds in terms of consuming their food, they can pick up harmful levels of lead too. So it went on beyond the waterfowl.

Now here's a field plot in Idaho. What these folks are doing, this is at a low water level time, they're going to take this mud flat and they're going to incorporate the pesticide, a phosphorous pesticide, a rather harmless pesticide, a fertilizer, a harmless fertilizer, phosphorous fertilizer into the soil. Fertilizer from agricultural tests had been known to bind up lead into a mineral; it's not soluble, it's not accessible to animals.

So they wanted to know, would that be one way to remediate lead-contaminated areas, something to mix a phosphorous fertilizer into the mud. Well, we had them dig up tons of this mud from the Coeur d'Alene River and we dried it out, we sieved it to get the stones and roots and if there were any shotgun pellets out of it. We mixed it into quantities into the diet that Nelson had said are realistic quantities.

We found out that if you mix phosphorous into the lead- contaminated sediment, you can reduce the toxicity by over a half. So that's one way to get at it.

So here is the payoff. In the past few years, courts have awarded 370 million dollars from the mining companies to repair the lead-contaminated Coeur d'Alene River. All those names, those 30-some names of scientists who worked for a good portion of the career studying lead, going back even to the lead shot studies (which are applicable in a way) were responsible for awarding the largest award, single award ever given for a contaminant case of this kind.

You can do a lot of good. That is a big payoff for the work of scientists over the years.

So okay, the lead shot thing, the tetraethyl lead thing is gone. We're going to clean up the Coeur d'Alene River hopefully. Is lead done? Well, probably not, because birds like loons have been known to pick up lead sinkers and they can be poisoned. You heard me talk about the lead bullets that can go out and get eagles and condors.

So, Barnett and Chris Franson (Chris used to work here) published this paper, it's a great big report, in 2008, talking about lead ammunition and fishing tackle still as a threat to the natural resources.

So the.., I think the moral of this story is that unlike the pesticides, heavy metals, you think you've got them nailed down and pinned down and conquered, but they don't disappear. They cannot disappear from the environment. And it's our activities which tend to bring it to the surface again and again and again. So there's like a constant vigilance that needs to be necessary.

Patuxent Wildlife Symposium
Thursday
Session Moderator: David Trauger

David Trauger introductory: Dr. Harry Ohlendorf

David Trauger:
Our next speaker is Dr. Harold Ohlendorf.

And Dr. Ohlendorf was employed at Patuxent as a research scientist, starting in 1971. And he also served here as the assistant director from 1973 through 1980. And then as the field station leader at Davis, California from 1980 to 1990.

His work focused on fish-eating birds and the bioaccumulation of contaminants through the various trophic levels.

Dr. Harry Ohlendorf:
Thanks, Dave. I'm going to be talking about contaminants related to Ag drainage, and this is something that emerged back in the 1980s.

So, briefly, drainage from Ag fields was being used for wetland management in California pretty extensively, without ever having been thoroughly tested for impacts on wildlife. So one of the goals of the Pacific Field..., Pacific Coast Field Station when I went out in 1980 was to do some evaluation of drain water, particularly, effects on birds.

So we began studies in the 1980s at Kesterson Reservoir, which is in the San Joaquin Valley, part of the Central Valley of California. And an important part of this is that it was integrated then with lab work here at Patuxent. In the course of that work, selenium was identified as really the chief chemical of concern in the Ag drainage water.

The integrated field studies at Kesterson along with the work that Gary Heinz, Dave Hoffman, and others did here at Patuxent has been identified as a gold standard in the field of ecotoxicology, and is often referred to because of the close connection that was made between the field work and the lab studies.

And through the work of Joe Skorupa and other people, some of our problems were identified in the Tulare Basin, which is the southern part of the San Joaquin Valley, and then across much of the western U.S.

And selenium has also been identified subsequently through the studies that have been done as a chemical of concern, resulting from mining activity, oil refineries, and coal-fired power plants because the selenium ends up being in the fly ash that is a waste product from the power plants.

Hopefully, you can see the gray areas that were identified as areas susceptible to contamination through the Department of Interior studies that Joe was heavily involved with looking at various areas across the western U.S. And then all of the green areas are areas where they're irrigated agricultural lands. Many of those are in areas that are susceptible to selenium problems and in many cases the drainage from those irrigated lands goes to a wildlife refuge or a state wildlife area.

A lot has been written about the work with selenium; *Ag Drainage in Kesterson*. This book chapter is one that coincidentally just has come out. And the request from the editors of the book was to... Well, they suggested this title. But the goal of the chapter, each chapter in the book was to describe the work that the author had done, look at one kind of actions were taken as a result, what were the conservation gains. And then unlike most of the things we write, what was your personal experience in being involved. So, I thought it was a strange coincidence that having received the invitation three years ago the chapter came out last month.

So, going back to where it all began, though. For some of you who don't know, before there was PowerPoint we did have slides that we could use in meetings and things like that, so I'm switching to old-fashioned slides at this point, with some help.

To go back to what we were doing at Kesterson and the field studies; so for the farmer the contaminant problem with Ag drainage is that if they don't have drainage... This is a cotton field; Color-IR shows healthy growth of the cotton plants up here but not over here. And the problem is salt accumulation in the roots under the plants. So, to solve that problem the farmers put in these subsurface drainage collector systems that discharge into a ditch to get this groundwater lowered to get the salt out of the roots now and discharge it.

And then what was done was that it was conveyed up a canal, discharged to Kesterson Reservoir, which is a part of the larger Kesterson National Wildlife Refuge, for disposal. And the plan was to continue this canal on to San Francisco Bay for discharge to San Francisco Bay, which has not happened, fortunately.

So, in the studies that we did at Kesterson, we used a variety of kinds of birds because of different migration patterns, food habits, and general ecology. Eared grebes were there in

one year. We had ducks, shorebirds including avocets, stilts, and then we also had Coots; I did stick in a picture that comes up in a bit.

But we collected birds, looked at what they had in their GI tract. So, in most cases what was left after the grinding and whatever was usually not enough for chemical analysis, but we could tell what they were eating. So we went out and collected samples in the ponds at Kesterson

ENDS (DVD/TAPE)

Patuxent Wildlife Symposium
Thursday
Session Moderator: David Trauger

David Trauger introductory: Dr. Charles Henny

David Trauger:
Our next speaker is Dr. Charles Henny.

Chuck worked at Patuxent for 21 years, both at Laurel and as the leader of the Northwest, Pacific Northwest Field Station in Oregon.

His research is *Emphasized Fuel Studies of Contaminants in Raptors*. And his talk focuses on how, when and why wild raptor population declined in numbers, with importance of Patuxent Laboratory field studies.

So Chuck, take her away.

Dr. Charles Henny:
Thanks a lot, Dave.

I was asked to talk about raptors and contaminants based on studies at Patuxent.

The first thing I did, I talked to Lynda Garrett; I said, "I need a list of all the raptor studies at Patuxent." She sent me a list that was 450 papers published, and 142 of those dealt with contaminants.

So what I'm going to try to do is synthesize some of this stuff and kind of bring it into an historical perspective as best I can.

The first raptor published study that I, that was on the list, was by Bob Stewart and others in 1945. And Chan Robbins was one of the co-authors that dealt with trapping techniques for hawks and owls. And if you take a look at the bottom there, that's Chan Robbins, a photo from a paper published in the Journal of Wildlife Management in 1945. So that's at least 66 years ago.
The other study or the other paper is of me 40 years ago. So things have changed a little bit.

The other notable long-term raptor study that was done at Patuxent started by Bob Stewart in 1943, and it was the red-shouldered hawks in the Patuxent River floodplain. And he published a paper in '49. There was additional data collected through '71 that I was involved in getting published in Ecology in '73. And then Woody Martin continued that study on his own time through 2004.

So there's like 60 years of data here on one species in the same study area, basically. And that population, as I summarize here, declined over the 30-year period that Woody was looking at it, a 87% decline.

But it was not related to contaminants. We did do some work looking at a few eggs in the '70s, and they were not really contaminated very much. It was basically a habitat loss issue.

The startup for the contaminant studies really began in the 1960; the '60-'66 period. And we had these tremendous raptor declines. Chan Robbins, again in 1960, compiled the bald eagle population information that was available from many sources, including Broley's data from Florida. It was a 20-year data showing a 50- 90% population decline, and with reduced productivity post-World War II coinciding, of course, with the modern pesticide era.

Also summarized was this Hawk Mountain migration data showing that the percent of immatures dramatically declined from in the '30s; from 38% immatures to 21% to 10% in '57-'58.

So, we've got a productivity problem in several areas with the bald eagle. This coupled with DeWitt's 1956 paper published at Patuxent showed pheasants and quail exposed to pesticides laid fewer eggs, produced fewer young.

So there was a big concern at that particular time.

There was a lot of work that was actually published through the Audubon Society with

DeWitt and Buckley in '62, and Buckley and DeWitt in '63, sort of updating the situation with bald eagles. They trapped a bunch of birds in Alaska, brought them to Patuxent for feeding studies. They started getting dead bald eagles in to necropsy and run contaminant studies, had it summarized here. All of the carcasses contained DDT, all of these 54. And they didn't know if the DDT burdens were high enough to cause problems. They also found DDT in eggs, but uncertain if it was a problem.

The next big event from a raptor standpoint was a Peregrine Falcon Conference in 1965 in Madison. It was called together by Joe Hickey. And Joe Hickey had ties with Patuxent dating back to his early studies with bird banding data. And he also did a Peregrine Falcon study in 1942, where he had 209 Peregrine eyries east of the Mississippi River. He had had some other folks, Dan Berger and others check 133 of those sites in 1964, none were occupied. So, it looked like we'd lost the Peregrines in the east before we even knew what was happening.

The same time, Derek Ratcliffe was at this conference, from the UK, and he told the same story about the Peregrine declines. But he had a little more closer observations; he had egg breakage, hatching failure, death of young, failure to lay. Plus he had some eggs analyzed and saw DDE, DDT, dieldrin, heptachlor epoxide, and he argued that these concentrations were sufficient to reduce breeding success.

At that same conference ospreys were brought into the picture, with similar population declines. Not as, not total losses, but reduced reproductive success and declining populations in Connecticut and Michigan.

This was, these were not Patuxent employees, but this is kind of the story. There was a roundtable discussion at that conference, which was published in Hickey's book, and it involved Joe Hickey, Ian Press from the UK, Lucille and Bill Stickel. And this kind of summarized what the situation was in 1965.

And you can kind of read the points there regarding birds may have a normal or near-normal reproduction with relatively high DDT concentrations in eggs. This was probably based on other studies of other species. And there was no evidence that a few parts per million of DDT in eggs caused reproductive trouble. OC/organochlorine doses that clearly reduce avian production are, with possible exceptions, not far below those that kill some birds. And long continued intake at lower doses was far more lethal than once thought.

These were kind of points that they were making. You can kind of... I'm not going to go through every one of these points here, but here's a picture of Joe Hickey and Lucille

Stickel. And the Stickels noted at the end, at least this was kind of their statement, 'Let us agree that pesticides kill wildlife and may cause population declines, but so do many other things.'

They were concerned about disease, metals, predation, a number of other things, and noted that much work is required to study these other possibilities, including combinations of pesticides.

And I think a key point here is that they recognized the tremendous difference in sensitivity among species. And procedurally, the Stickels noted that these unhatched eggs, when they're analyzed on a wet-weight basis need to be adjusted for moisture loss.

And they were saying, "Hey, there's a lot of inflated egg concentrations out there in the literature and we need to not present those biased high concentrations, and present in a way to adjust for those factors."

And this is still an issue today; there's still biased egg concentration data out there when there's old eggs collected that have been dehydrated and not adjusted for moisture loss.

The other point that they made was that they concluded the lab studies that brain concentrations were the key and the other organochlorines anyway and other tissues are too variable. And both Stickels downplayed the importance of egg breakage in 1965 and noted that it was not uncommon in captivity, even with controls. So they were a little suspicious of Ratcliffe.

They updated their concepts, Stickel did, in a paper the next year at the North American and said, "Hey, the transfer of..., from adult to egg is well known."

There was still some concern about concentrations in eagle eggs that may indicate an adverse effect. It's not far, it's far from clear. And pointed out that gull eggs and pheasant eggs hatched with much higher concentrations than they were seeing in the eagle eggs. But they did note that exposure to eagles to DDT in dieldrin was nationwide, and that an occasional eagle obtains enough dieldrin and perhaps DDT to place it at risk.

And the other thing they mentioned was that most of the eagles that die in the U.S. die of other causes than pesticide poisoning. I'm not sure that that's a good point to try to make here, but perhaps these birds that are dying of pesticide poisoning are not being found by people.

Sudden lethal effects on behavior cannot be answered yet, so.

This was the situation in '66. Their plans; continue monitoring eagles' eggs and dead adults. They extended analysis to some important heavy metals, began the food chain investigations, understand the residues and tissues in eggs better, and they'd already established a kestrel colony to test reproductive effects.

These, this summarizes the raptor papers. The red lines show the contaminant papers. And you see a rapid increase in publications starting in about, in the late '60s, and sort of peaking in the early and mid '80s, and then dropping down quite dramatically in the '90s and 2000s. Part of that is that in '93 Patuxent lost many other field stations. And that work continued, but was basically no longer under Patuxent's umbrella. It was..., these publications were associated with some other center.

So, at the time of the peregrine conference, eggshell thinning was not known. Just all of these world-wide declines of osprey and peregrines and bald eagles and many other species.

Well, Ratcliffe went away from that conference with egg breakage on his mind. And he talked to a colleague, a British colleague, and this guy, Nethersole-Thompson, suggested that he look at eggs in collections to try to understand if there were some changes in these egg shells. And that was really a key to understanding this whole issue.

So Ratcliffe designed this thickness index where he couldn't get a micrometer through these small blowholes that these egg collectors had, so he designed this index that sort of went around that problem.

The interesting thing is that he was in constant contact with Joe Hickey, who had the Patuxent connections. Hickey basically got his grad student funding through Patuxent to check all of these museums in the United States and collect eggs and measure them. And he devised an egg micrometer, modified it to actually measure the eggshell thickness.

So, when Ratcliffe published his paper in Nature in 1967, that was a point where most of the people in this field, at least me, I can remember where I was when I first heard about that paper. It was kind of like, 'Where were you on the day Kennedy was shot?' I mean I think it had, it had that much of an influence on what was going on. And then within a year Hickey and Anderson, based on Patuxent funding, reported the same kind of eggshell thinning in the United States, published it in Science.

Well, the next step at Patuxent really was 'How do we solve this problem with understanding what's causing this eggshell thinning?' And of course they had the colony,

the kestrel colony already started. They weren't really thinking about eggshell thinning, but to modify their studies to include eggshell thinning was really not a serious problem. So by 1969 and 1970, Porter and Wiemeyer and Wiemeyer and Porter published their classic papers in Science and Nature, showing the eggshell thinning and reproductive effects under controlled laboratory conditions.

So, really much of the work at Patuxent that the Stickels designed and developed was pretty much set up prior to 1965, with the slight modifications for the eggshell thinning. This is the way I kind of see it anyway. And the staffing up after 1965, and there are many of them, many people in this audience that got hired in on the late '60s, early '70s to fill in the missing parts to this whole contaminant story.

I'm just going to show a little bit of information regarding how the bald eagle carcass data was used. Now they collected hundreds, you know, over time. And this is kind of a summary of many reports that were multi-authored by many chemists and you can see the importance of dieldrin as a percent of the population that was sampled, or the dead birds that were sampled. The use of dieldrin peaked in the late '60s and was banned nationwide, except for a few minor uses by 1974. So you can see how this big data collection at Patuxent, the percentage of deaths from dieldrin dropped from 13 to 6.5 to 3 to 1.7%. So following that band, the importance of it minimized quite dramatically over time.

The kestrel lab studies that started out with Porter and Wiemeyer's work with DDT and dieldrin were used for many contaminants, and they're listed here by year. And the senior authors that were involved in these, if there was more than one paper I listed the authors names down at the bottom. But you can see all the different contaminants by year that were subjected to laboratory studies with the kestrel colonies.

Eastern screech owls and barn owls were also part of the lab studies at Patuxent. You can see the contaminants there; the SO (screech owl) and BO (barn owl). Not as many studies, but a number of different contaminants that were looked at over time. That's Vivian Mendenhall with the barn owl.

I think from a field perspective, the studies that dealt with trying to understand the effect of egg residue concentrations on raptor productivity were some of the more important ones that were done at Patuxent. And Wiemeyer was the senior author in essentially all of those (add the: 35:20) heptachlor work, which I was involved in. That was a field study where we had hundreds of nests.

But these papers provided a way for people in various states to go out and sample a series

of eggs from their population of interest, for at least these species, and say, "Hey, is this a problem or is it not a problem?" Based on what was reported in these contaminant studies summarized through Patuxent.

I feel like the combination of the lab studies and the field studies at Patuxent were one of their most important contributions. And at the time, in the late '60s, the peregrine essentially was gone from the east. We did do some work along the Atlantic coast and the Texas coast, trapping birds and migration and looking at contaminant concentrations in blood. And we did that over about a 20-25 year period, until the residues dropped to virtually nothing in 2004.

The bald eagle was in low numbers. It's very sensitive to human activity near the nest. And, of course, as I showed in the summary, Patuxent sort of focused on the carcasses and the addled eggs that became available, and really didn't do too much in terms of studying nest success in the field.

Okay, so that leaves the osprey as the obvious candidate for detailed field studies; less sensitive to human disturbance, quite visible, easy to study. Lots of information was known about their, was being learned about their natural history and various characteristics. Population number data was accumulating.

This is kind of a busy slide, but this summarizes the situation as of about 1975. The information here, the subtotals, this is Gardner's Island, Connecticut River, Rhode Island, the top three. We have a continuum of data, and you can kind of see how these numbers changed and what the annual percent change was over these five-year periods. And, of course, DDT was banned in here, but the peak use was somewhere back in here.

So you can see a little bit of recovery, but I mean this was pretty dramatic. The productivity was terrible in these years. I mean some years really low.

So, I was involved in some modeling out of the Migratory Birds Population Station using banding data to try to understand how many young should be produced. And we did that and came up with these numbers of .95 to 1.3 young per nesting pair. And then Paul Spitzer, who's actually in the audience today, used this same model to calculate the population changes and revised this thing down to a lower rate of .8 young needed to maintain a stable population.

And the other interesting thing that Patuxent did was they exchanged eggs between Connecticut, where there was low production, and Maryland, where there was higher production, to determine if productivity was caused by something external to the eggs

like food supply, predators, human disturbance. And sort of the bullets down below, Maryland eggs incubated, the Connecticut hatched at the normal rates. Connecticut eggs incubated, in Maryland no improvement. Of course, the Connecticut eggs 18% shell thinning, Maryland eggs 10%.

So that all kind of fits that it was the egg.

A cartoon of the day actually about this Patuxent experiment; now we've got DDT up here in the old biplane, you know, flying over, I got you on the decline of 10- to 14% annual rate. And then here's this guy carrying an egg in from Maryland to try to help the situation. And that's, that was early 1970s cartoon stuff.

To give you the longer term summary, this is some data actually from the Pacific Northwest looking at eggs collected between '72 and '86, which was, you know, basically the earlier post-DDT era. The samples, most of those numbers, the open circles, are down here with the higher concentrations in the thinner eggs. This is kind of the intermediate group that are half and half in here. And in the later time period, when things got back to normal, normal thickness is about .55-mm.

So that just shows the recovery that occurred based on a large series of eggs that were collected in Oregon, Washington, and Idaho over about a 35-year period.

In the early years, when I was at Patuxent in the early ‘70s I was actually not with Patuxent, I was with the Migratory Bird Station, Bird Population Station. And the situation there, it was a really dynamic time to be employed at the Center. There was Fish and Wildlife Service pilots and planes available for non-waterfowl surveys if there was no timing conflict with their waterfowl surveys. I mean these guys wanted to fly.

And so we developed a study plan to survey Chesapeake Bay for ospreys. I mean the whole bay. I mean we had a number of planes and observers, and we did that in 1973. We set it up just like a waterfowl survey. It was a double sampling procedure where a portion of the area was sampled on the ground, and with all the area sampled by air so that we could get air-ground adjustments and come up with total population estimates.

And interesting I think, I talked to Chan Robbins and I talked to Alexander Wetmore, who were two guys that know more about birds in this region, the Chesapeake Bay, than anywhere else. And I asked them, I said, "How many pairs of ospreys do you think are nesting in Chesapeake Bay?" And the numbers they came up with, well 200 to 400 pairs. And of course when we went out and did the survey, there were1450 pairs.

And this was the basis, the benchmark, you might say, for Chesapeake Bay. And as I show here, by 1995-'96, another survey was done, and there were about 3500 pairs nesting in Chesapeake Bay. So that 1450 was probably a low point.

And then the survey was extended from New York City all the way to Georgia. And then when I went west, we did an Oregon-Northern California and Baja-Sonora-Sinaloa, those areas.

And another interesting story I want to relay, the..., you wonder why the cypress? When I was down in South, in North Carolina in 1974, I flew over Matta... Lake Mattamuskeet on Mattamuskeet National Wildlife Refuge, and there were 38 pairs nesting out in this lake in these little cypress, some of them no more than 15 feet above water.

And I came back to Patuxent and I was casually talking to Fran Uhler, who's been mentioned a number of times here, and I said, "I saw these nests in these little cypress out in Mattamuskeet." And he says, "Well." He says, "I went down there in 1928 to look at that area as a possible refuge site. And it was purchased, I guess, under Reconstruction in 1936 as a national wildlife refuge. And prior to that time period, it was drained and the agriculture situation had failed and it became a refuge in '36 and it was re-flooded." He said, "If you run an increment borer on those cypress, they're all going to be 30 to 40 years old." And we did that and he was exactly right.

So the amazing people that were at Patuxent with the background to provide that kind of insight was amazing to me.

We did the similar types of surveys in Mexico, and the population was down in the '70s, but increased and stabilized in the '90s and 2000s.

The osprey was such a good species to study that by 1981, we could come up with a total population estimate for the United States, which was about 8000 pairs. It jumped to 14,000, about 14,200 in '94, and about..., the data was not as tight in 2001, because the effort to study the effort had dwindled quite a bit, but there was still an apparent increase by 2001.

So, the ospreys have played a big role, I think, in these contaminant studies. But they're also still playing a role with the emergent contaminants; the flame retardants, the herbicides, the perfluorinated acids and sulfate compounds. And they habituate to man, they're in some of the most god awful places to nest, but they handle it. And so if you're concerned about nests associated with a big city or a big estuary, they're the species to look at. They've got a pretty wide range of distribution. The other thing is they've got a

worldwide distribution. So you can compare what's going on in your part of the world with what's going on, say, on the Volga River in Russia or the northern tier of countries in Europe or Japan or Australia, without being concerned about species-specific differences.

I want to end with one story that talks about how the lab studies and the field studies at Patuxent in those early days kind of make a lot of things happen. I was, I sent a memo to Patuxent in May of 1977, that there were dead magpies at a livestock feed lot where Warbex (or famphur) was used as a pour-on. This raptor rehab, or actually he was a Oregon Department of Fish and Wildlife employee, picked up these dead magpies and put them in his freezer. I think he was saving them for me. And he was gone one weekend and his wife was out of food for the owl, so she gave him, gave the owl one of these magpies and it died immediately.

So I sent this story into Patuxent. As a result of that, Woody Hill and Vivian Mendenhall fed some barn owls famphur-dosed quail and saw this significant cholinesterase inhibition and concluded that owls could succumb to secondary poisoning.

So after that happened we did a field study in Washington in 1982, and the photo here is of a squeeze chute, where we actually poured the material on the backs of the cattle at the recommended rate. And we treated 535 head of cattle on seven ranches and monitored what happened essentially on a daily basis. Magpie started dying the day of treatment, and they were still dying 100 days later. And we had sampled the cow hair at weekly intervals for 100 days, and we saw a decay curve, but there was still contaminant famphur on these things 100 days after treatment. In addition to all these magpies that died we had a red-tailed hawk that died eight to ten days after treatment. And guess what it had eaten, it had eaten a magpie. And we had another sick one that we just picked up, and it had cholinesterase inhibition.

And the story gets even more interesting; in the bald eagle story, or the carcasses that I talked about earlier, there was always a lot of open cases. You know, we don't know what killed this bird. And Franson wrote a paper in '85, and I was involved in a paper in '87, where we looked at a bunch of bald eagles that had died. And lo and behold, in a number of states, in a one-year period, there were eight bald eagles from Iowa, California, Idaho that had eaten livestock carcasses directly. Or we had red-tailed hawks in Oregon and Colorado that ate magpies. And we had this Great Horned Owl which was really unique; the Great Horned Owl had eaten on a red-tailed hawk that had eaten a magpie. So we really had tertiary poisoning.

And this was all brought about because we..., Patuxent had field stations, it had

chemistry, it had laboratory capabilities, and that all kind of made all of this stuff happen back in those earlier days.

And I will leave it with that.

Patuxent Wildlife Symposium
Thursday
Session Moderator: David Trauger

David Trauger introductory: Dr. Elwood Hill

David Trauger:
You mentioned Woody Hill's name, and that's a good segue to our next speaker. Dr. Elwood Hill was an ecotoxicologist at Patuxent for 26 years, and focused on studies on wildlife.

He developed many laboratory techniques that are now used widely by EPA, universities and others to study wildlife toxicology. And he's going to share some of that experience with you. Woody.

Dr. Elwood Hill:
Well, at this time of day sometimes you need a little humor, and Chuck just provided the fodder. At the time we were doing that work regarding magpies and so forth, we looked at a lot of different things because they'd use the back pour. And of course the back pour could bind up in a hair on the back of the cattle, and it was absorbed and had its useful purpose there they factored in. And of course a lot of it came out in the feces.

So we had a secretary in the chemistry section at that time by the name of Marian Kramer, and she asked me one day about this project that Chuck was doing and I was kind of collaborating in and we'd gotten some samples. And she wanted to know what the samples were. Well, it turned out in addition to the hair some of it was feces. So she dubbed Chuck 'The Prince of Poop.' And that was sort of the highlight of that year, I think.

Now on to what I'm supposed to be talking about here, my topic is really pretty simple, and it's going to be brief, even though some people think I can't be brief. Anyway, I wanted to make it very clear because some people said, "Well, now why did you leave Patuxent?" And of course I gave a number of different answers, but I really didn't leave Patuxent, I just got lazy and thought I would return to my roots. And once you're a desert

rat, always you're a desert rat. And so there I am among the bears and rattlesnakes and things of that nature that I enjoy immensely.

And so I'm back for the first time in 16 years to this center. Now there's two ways I can handle my presentation, three ways really; I can get very elaborate and put forth a lot of detailed graphics that would bore everybody to tears including myself, or I could simply read this book for 15 minutes, or about 12, and it would cover my talk. And it was for a very interesting reason, which I will get into as we go along, explain why I say this. And it says a lot about the Patuxent Wildlife Research Center, the innovative activities of the people here, the interactions, and how important this center truly is to the science of wildlife toxicology, environmental hazard, and other environmental issues in addition to other ecological considerations.

And the title of this talk of course as you saw from the program is *Standard Procedures for Wildlife Toxicology Studies*. And that's precisely it. But it does have a bit of a history, and most of what I'm going to say probably has been alluded to earlier today by somebody or many people.

But historically, as mentioned earlier, prior to the Second World War, there was comparatively little interest in the concept of wildlife toxicology. I don't think it was anything more than a passing thought if at all.

But one name that hasn't come up today is that of Aldo Leopold. I kind of looked upon him, I think a lot of people of my generation did, as the prophetic icon of wildlife science. And he actually envisioned possible problems from agricultural pesticides, industrial effluent from mining, manufacturing, logging in the 1930s. This is reported or alluded to at least in the Sand County Almanac, which I suppose most of you, most of at least, used to have mandatory reading years ago. But he saw these things happening and it was published in the ‘50s. There were a few states, such as California and Michigan, and of course, the U.S. Fish and Wildlife Service that might have been aware of some problems, especially fish kills. It seemed that kind of led the way, a little easier to document a lot of times. And I think that the fishery scientists back in those days probably had a little more insight and interest into what was happening to the environment simply because of the fact that the fish were, you know, in the puddle and the puddle was poisonous. Wildlife can disperse and their difficulty, of course, fish are wildlife.

And so anyway, it seemed that most of the information in those days was just pretty much anecdotal. And a lot of it had to do with people started imaging that there would be problems with agricultural pesticides and heavy metals just on the assumption the hazard had some degree of likelihood, even though this wasn't particularly well documented,

people really weren't getting into it too much from a scientific, or from the chemistry perspective.

Anyway, it turned out that very, very little work was actually done with experimental circumstances with wildlife species. In fact, most of the studies that were done back in those days had a medical orientation to them. And what we glean from the literature, as I understand it, was mostly information on the toxicology of certain chemicals to laboratory rats and rabbits and so forth because it was allied to the issues of the medical industry.

Then in 1962, many times this has been mentioned today, Rachel Carson's blockbuster hit the streets. It was a best seller and so forth. And it raised a lot of concerns about pollution, the effects of nature as we know it. But the interesting thing is the fact that most of it was anecdotal. And I might add, even though I'm a little disappointed because as I went through *Silent Spring* for another purpose here a few months back, I remember discussions with the Stickels and Dr. Dustman and a few others about having met with Rachel Carson on various occasions and talked about issues that were to be put into her, I guess, planned book at that time, which of course planted a big seed in the Stickel's mind also. But I went through *Silent Spring* and I could not find Patuxent listed. But I do want you all to know that Patuxent was implicated in *Silent Spring*. And it was in my estimation, you know, maybe I've got an attitude problem, but as far as I'm concerned, it's probably one of the most important books that's ever been written because that was what caused the change in the directions that we were going.

And on a side note, *Silent Spring* directly brought me to Patuxent. I knew nothing about contaminants, still don't know much about them, but it turned out that a guy that I worked for in Savannah, Georgia with U.S. Public Health Service was a quail hunter. He read *Silent Spring*. He thought there must be a problem here; it's going to ruin my quail hunting, so I'd better get a biologist on the staff to do some work on toxics. And so he hired me. You know, I don't think he knew much about what he was talking about and I certainly didn't have any idea what he was talking about or what I was talking about. And so, you know, time carries on.

Anyway, it was, it was interesting because I showed up in Florida for this particular meeting with these people at my new job. And one week later I was at the Patuxent Wildlife Research Center sitting down with Dr. Stickel, Bill Stickel, Dr. Dustman, other people here, Bob Heath, any number of people who were on the staff at that time, talking about how I might design some experimentation to look at the effects of the Public Health Service spraying backyards in domestic settings with DDT to runoff. That was my first job. And I was coached continuously by the Stickels, particularly Bill Stickel in

this particular instance, and it was good.

But anyway, it really got me going. And then in the '60s, of course, this was in the mid-'60s actually, much of the wildlife toxicology was simply anecdotal as I mentioned, highly generalized science. And alongside of the issue that I mentioned with the laboratory rats and rabbits, the Denver Wildlife Research Center, for mostly other purposes developing animals for their predator and rodent..., or animals..., chemicals for their predator and rodent control program, they were starting to generate data on acute toxicity mainly. And they were using a simplified test of about 20 animals to generate an LD50, and they were looking as an accelerant to this whole thing; they were really looking mostly for chemicals that would kill predators and their rodent control and birds that were involved in damage.

So it was all kind of interesting from that standpoint. And Patuxent though, the Stickels primarily, and Bob Heath, sat down and decided that this acute type test wasn't really the answer to what we see in wildlife studies, or wildlife situations.

They generated a different study, which we eventually coined the term subacute. It was a five day trial where birds were exposed to potentially lethal concentrations that were intended to surround the LD50, perhaps somewhere from the LD10 to the LD90, or LC (we called them either lethal concentration rather than dosage). And that study was published first in the Fish and Wildlife Service Report in 1964, where they talked about this concept and study. And that was well received.

They also generated the basis and protocol for doing a reproduction study, which Chuck Henny eluded to a few minutes ago, with mallards, where they were able to tie DDT, primarily its metabolite DDE, to eggshell thinning in the laboratory. And that was really pretty good. And that particular study published in Nature in 1969. It really got a lot more interest going, and, of course, coupled with these raptor studies that Chuck was talking about it started bringing this picture together.

And their first study, however, based on their design, was based on a series of pens of animals; one or two males, either pheasant, mallard, or northern bobwhite, and a harem of females. And they did their routine studies and followed them through their breeding cycle. And then the young that were hatched via incubator were then observed for at least 14 days to see if..., I think it was 14 days, is that right, Gary? Yeah, they were observed. And they generated information on environmentally plausible concentrations of the chemical that you might find under most circumstances of different kinds of treatments and so forth around here, around the country.

And so this, this study flew and people liked it. Well, not long after that, along came the Environmental Protection Agency; it had been formed. And I might add, nobody mentioned that during the historical coverage of this stuff earlier, but Lucille was really getting uptight because when they were forming the Environmental Protection Agency, they wanted to take the entire contaminants programs from the Patuxent Wildlife Research Center and plug it into the U.S. Environmental Protection Agency.

I think between Lucille, I guess Dr. Dustman was still here at that moment, but they really put forth an effort to deter such a thing. As it is, we lost a group that was evaluating pesticides that was, they were at Patuxent. And I remember they were housed in Merriam Lab, or in Stickel, but I can't remember, I can't remember what that group was called, but they did go to the Environmental Protection Agency. They were a review group. They were very helpful. And they constantly referred back to Patuxent scientists, acquaintances, colleagues, and so forth to get the information or to get input into information that they needed to generate for the Environmental Protection Agency's initial efforts.

Well, of course one of the things they did too, was to tell the people at the Environmental Protection Agency, the new people just starting up, this was in the early 1970s, I'm sure Gary was involved in it, I was and a number of other people of course, Bill and Lucille. And the EPA folks came out and chatted with us about experimental design, what kind of protocols might be desirable for registration purposes.

Well as it turned out, they really took us seriously. And they did use the methods that were gen..., developed by primarily the Stickels and Bob Heath; he was our statistician who ultimately went down to the Environmental Protection Agency. It was unfortunate because he was a wildlifer I believe, from Michigan State, and he really had the concept of field biology in mind and he was a statistician. So he was helpful.

But all of this went out to the efforts of the EPA and this test, the subacute test that I'd mentioned, the feeding trial, the five-day feeding trial, the reproduction study; the basic protocol was incorporated into the early phases of at least FIFRA. That's the, what the, oh, one of our major acts, I can't figure out what the acronym stands for but I know Gary knows it off his head.

And we worked with also ASTM (the American Society for Testing Materials) and Gary was very, very much involved in that and got an award from them, at least an acknowledgment of all of the contribution he had done in helping them design experiments to review different protocols and so forth.

So there was the Patuxent pie.

And also at that time, in reviewing some of these things, Gary had the wisdom and foresight to modify our reproduction studies. So instead of going with groups of animals and then following maybe the development of the young for a few days after hatching, he came up with pairs. And he did a real nice job on coming up with paired studies and natural incubation, and still doing perhaps under some circumstances, a little bit of incubation in the artificial incubator for other reasons.

And of course that brought then Dave Hoffman onto the scene, and Dave took all of these eggs from the incubators and did all kinds of crazy things with them, you know. He dosed them via the air sac, he did other kinds of things, he used a number of topical treatments and so forth. And although EPA never really took that particular, you know, work on the egg studies that Dave did, the embryology, they didn't take that and put it into their regulations. But, a lot of people have done that work and they've followed up on that.

So, from the standpoint of standard toxicity testing, we at Patuxent really fed the scientific community an awful lot of information. And I think for that, we have to really say hello and thank you to Bill and Lucille Stickel, Gary Heinz, Bob Heath, Dave Hoffman, because they really made a contribution that is critical to the environmental sciences, and particularly to wildlife toxicology.

Thank you.

Regarding this book, the whole theme, or half of it critiques the work that was generated in 1964 by these folks. And this was published in 1992. They still haven't gotten it resolved, but what they've done mainly is change from looking at these chemicals and some of these experiments from the perspective of the statistics rather than the realities of the world out there. They're fine tuning, they can't really seem to find anything wrong with the basic concept. So I think that we should be proud of that.

Patuxent Wildlife Symposium
Thursday
Session Moderator: David Trauger

David Trauger introductory: Thomas Custer

David Trauger:
Our next speaker is Dr. Tom Custer.

Tom was a research biologist at Patuxent, at Laurel, and in Texas, and Wisconsin over 20 years, from 1974 to 1994, where he worked on contaminants of colonial nesting birds.

He... his presentation is going to cover the work that he has continued to do from the 1970s to the present.

Dr. Thomas Custer:
Thanks Dave. So I was at Patuxent from '74 to '83, and then moved down to Victoria, Texas Field Station '83 to '91, and then finally to Lacrosse, Wisconsin. I was with Patuxent from '91 to '94, and then as you heard several times, we changed and I got into the Upper Midwest Environmental Science Center, where I am now.

Talking about colonial waterbirds and contaminant research, and colonial waterbirds would be herons and gulls. Well, the long-legged wading birds and gulls and terns, herons, and mallards, and work we've done with contaminants here. So, the approach is a little bit different, a little more, maybe some more data.

Why colonial waterbirds? I'll go through a brief publication history to give you an idea of the volume of things that have gone on, but maybe not the details. Emphasize some of the early activities and highlight some specific studies. It's going to be a real specific approach rather than overall.

So why colonial waterbirds? Well, in the 1960s the double-crested cormorant (and you see up in the upper left standing by a nest) were extirpated from Great Lakes, Michigan, and Superior. Black-crowned Night Herons; there was estimated to be a 90% decline population on Long Island, with over 20% eggshell thinning. And brown pelicans up to 50% eggshell thinning. And here's a picture of Jim Keith, who worked with us for a couple of years, when he moved from Denver Wildlife Research Center, and a couple of his friends, immature brown pelicans.

Here's a picture of Don White on the Gulf Coast, some are collecting gull eggs. And, of course, a picture of Chris Mitchell on one of the islands, collecting skimmer eggs, now Chris Custer.

As far as a look at the publication history of colonial waterbirds, I went, I didn't go to Linda. I should have gone to Linda, I went, actually, to the website of Patuxent and I just

copied everything that had to do with colonial waterbirds and did some subcounting as far as contaminants. And it's not exactly accurate then, but it gives you a feeling for the intensity of effort that went on between early 1970s and 2010.

These are first authored publications, so that'd be Larry Blus, who was one of the first, and I'd talk of some of his studies. Chuck, Harry Ohlendorf, Kirke King, Don White, myself, Gary Heinz, Hoffman, Barnett Rattner. And the number of publications is on the left with their senior author on, and the time span that they were, these publications occurred, is across the horizontal.

You'll notice that I was publishing things after 1994 as senior author. And that's because of the cooperative relationships with folks here at Patuxent such as Dave Hoffman, Mark Melancon and others. And I continue to work with folks at Patuxent.

And there are other individuals who published fewer than five publications.

So, in similar way to the raptor publications we had, you know, well over a 150 studies on colonial waterbirds and contaminants since the early 1970s.

In the early 1970s, Harry Ohlendorf attempted to establish a heron colony here at Patuxent. And he collected six species from Fisherman's Island and Bronx Zoo. This is a picture of Fred Kreitzer out at one of the colonies holding an immature Black-crowned Night Heron. The other species involved were Great Egrets, Snowy Egrets, (Simple: 1:16:05) Herons, Tri-Colored Herons, and Cattle Egrets.

There is Fred loading up some birds, getting ready to haul them back to the center.

This is an example of one our pen facilities. And birds inside the pen there are adult Black-crowned Night Herons, several adults, immature Black-crowned Night Herons, and Great Egrets in some of the pen facilities.

Also in the early 1970s, as Chuck alluded to with regard to the osprey, we didn't know where all the heron colonies were. So one thing I got involved with along with Ron Osborne (and this is a picture of Ron checking out surveying positions on a heron nesting in the colonies) we did, we organized a survey of colonies along the Atlantic coast, from Maine down to Florida. And all of these dots were the colonies that were identified. There were some 262 colonies and over a quarter of a million breeding birds that were found in that particular survey for two years.

The other thing that came of this was we got involved, myself, Kirke King, later Harry

Ohlendorf, Jim Kushlan all got involved in the formation of the Colonial Waterbird Society. I won't go through the details of all the conferences, but what it has resulted in, right now we have a very active society and journal, it's called *Waterbirds*. There are 30 Water volumes, and we've got a couple past presidents in the audience, Mike Erwin and Jim Kushlan.

One thing that Harry did in the early 1970s was organize a collection of heron eggs along the Atlantic coast. And it's not only Black-crowned Night Herons but several other species. And this is the result of the Black-crowned Night Heron egg collection; there's a picture of Harry, I think this is on actually the west coast, while marking the nest of a Black-crowned Night Heron in a clutch of eggs. Basically, he collected one egg from clutches of each of these colonies. This is the concentration of DDE; notice that it's on a log scale so that these numbers are actually higher (unclear: 1:18:25). And these are individual colonies from in this case Massachusetts, down to Florida, and around into Louisiana.

Now the take-home message of this, when you look at these mean concentrations is that the concentrations were much higher in the north than in the south from his collection. And one of the concerns at that time was that pesticides were still being used, DDE was still being used in Mexico.

And so one hypothesis, which now seems awkward, is that these birds in the north, maybe they're differentially going to Mexico and picking up those contaminants, and that was actually testing using the data from the Bird Banding Laboratory, which we've heard about earlier today. Harry Ohlendorf looked at birds that were banded in northern states, and whether they were recovered in North America or Latin America, and birds banded in the southern states the same.

And if the case had been true, you expected more of these birds down in Latin America, but it was not. The southern states, 20% were recorded, reported in Latin America, and only 8% in the northern. So that idea was discarded.

Larry Blus was one of the first to use the sample egg method, among the first. And basically, a sample egg method is you take one egg in the clutch and then you look at the success of the remaining eggs in that clutch. They're still using that method today.

Larry did this with brown pelicans on Cape Romain National Wildlife Refuge in South Carolina. And basically, he found that when you look at successful nests and unsuccessful nests, DDE in brown pelicans was significantly lower in successful nests than unsuccessful nests. And the same goes for dieldrin and natural PCB's. So he did

find an association between DDE and nest success.

The other thing of interest is that the concentration that he has on the papers, which demonstrate this further, was about three-parts per million, which relative to other species is rather lower.

END DVD #4; DVD #5

Dr. Thomas Custer:
.. Well, in this case, at about 8- 12-parts per million of the sample egg, only 58% were successful, shell thickness was lower and percent of cracked eggs was elevated. Again, using the sample egg method.

Another study that Chuck got involved in, which was interesting, was the fact that at Ruby Lake they were high DDE concentrations, lower reproduction, high incidences of cracked shells, but no indication of breeding ground contamination.

At a colony in Idaho there were lower DDE concentrations. So what Chuck and Larry did was to put transmitters on both of those populations and followed with by that particular airplane along the Mexico coast and down into southwest U.S.

Well, it turned out that the Idaho Night Herons with the lower DDE concentrations wintered primarily in coastal Mexico, whereas the Ruby Lake Night Herons with the higher concentrations wintered in southwest U.S. and some interior of northern Mexico.

Again, the same concern was that Mexico was using DDE as a pesticide, and this was not the case, at least in this study.

One of the assumptions of the sample egg method that Larry started in the early 1970s with colonial waterbirds is that if you take that one egg from that clutch that it represents what's in the other eggs of the clutch.

So Harry Ohlendorf had a series of fall clutches that were available, and working with Grey Pendleton, the statistician here at the center; Grey basically took a random egg from the clutch and then another egg random from that clutch, and looked at correlating and did that simulating that time, that several hundred times. And this is an example of one of those regressions. Basically suggesting that if you take an egg, and this again is on a log scale, if you take an egg randomly from that clutch, it does represent concentrations in other eggs in the clutch, supporting the sample egg method.

One thing that was one of interest in the Great Lakes were that PCBs were implicated in egg mortality. These are egg..., these are mean PCB concentrations in colonies, and this is the egg mortality associated in that colony in a particular year. And this is a Double-

crested Cormorant nest. And here's the "poster child" with a crossbill, reportedly associated with PCBs.

Well, I attempted to do a more detailed study on this using the sample egg method and to try to pin down the exact relationship at a particularly contaminated colony. And when we did that, to our surprise, and we looked at (the earlier study didn't take into account the other contaminants by the way, only PCBs), when we did a logistic regression, incorporating all of these variables, PCBs we thought was the culprit, fell out. It wasn't significant. Dieldrin fell out. DDE was significant, suggesting that DDE was still the culprit with regard to at least Double-crested Cormorants in the Great Lakes.

And this is a relationship with hatching success in relation to DDE concentration in Double-crested Cormorants in Green Bay, Wisconsin.

One of the last studies I want to go into a little bit of detail is one, it's a long-term study, which we had very few for colonial waterbirds, about the long-legged wading birds we have very few too. Terns and gulls there are several studies.

This is one on Black-crowned Night Herons and Snowy Egrets on Alcatraz Island in San Francisco Bay, and it's been conducted by Roger Hothem, who was mentioned earlier by Harry, since 1990 to 2011. Of course, you know, Alcatraz is known as "The Rock."

And there's a heron incarcerated at the "The Rock." This is the colony itself. Here are the sub colonies around Alcatraz, and a Snowy Egret colony.

And Roger's been looking at collecting reproductive success data too, but looking at population trends on the colony through time since 1990. Here's when the Snowy Egrets started to show up.

And here's a picture of Roger on the island checking one of the nests.

And finally, you know colonial waterbird studies continue at Patuxent. Barnett Rattner on the top and Gary down below are working with Common Terns. Basically, collecting fresh eggs and injecting those with one of the new emerging contaminant studies that was spoken of earlier by Chuck and one of the PBDEs (flame retardants), and looking for concentrations.

Right now we don't have a really good handle on concentrations of PBDEs, flame retardants, and how they affect hatching success and reproduction in terns.

I want to thank Harry, Roger Hothem, Barnett, Chuck, and Chris for pictures for the presentation. Thank you.